PROJECT MATTERHORN

Princeton University
Princeton, New Jersey

Report completed at
Los Alamos Scientific Laboratory

Report written:
August 31, 1953

PM-B-37

FINAL REPORT OF PROJECT MATTERHORN

Work done by:

All members of Matterhorn staff (names listed in section herein entitled "Matterhorn History") with the collaboration of the members of the Los Alamos Scientific Laboratory, the New York IBM staff, the Washington SEAC group, and the Philadelphia UNIVAC organization.

Report written by:

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Princeton University, and on June 28, 1951, Subcontract SC-6 of Contract W-7405 Eng. 36 was signed between the Regents of the University of California and Princeton University. Work began on May 25, 1951 in the Matterhorn premises, a guarded steel building 2 miles from the University campus at the newly acquired James Forrestal Research Center.

Personnel Recruiting

Beginning early in March 1951 letters were written to all known suitable U. S. theoretical physicists and applied mathematicians - a total of over 140 people.— requesting participation in a program of assessment and primordial design work on thermonuclear weapons. Reasons cited for the request were: (1) The difficult world situation and the need for assuring "peace through strength." (2) The importance of atomic weapons — and therefore of conceivable thermonuclear improvements — in the overall picture of U. S. war potential, relative to all other means of exerting power. "At peak production during War II we turned out about 4 kilotons a day in conventional high explosives. In the crude and highly arbitrary measure of total energy release this output means one fifth conventional atomic bomb per day, or in 700 days, 140 old fashioned atomic bombs. For comparison take any newspaper guess as to atomic bomb output." (3) Dismaying shortage of people on the idea assessment and primordial design end. "You would make percentage-wise more difference there than anywhere else in the national picture."

Matterhorn recruiting was cleared through Los Alamos to avoid interference. The following is a passage from a typical recruiting letter: "By carbon copy of this letter to Dr. Carson Mark, head of the Theoretical Division of the Los Alamos Scientific Laboratory, I am notifying him of our request for help in our program of assistance to the Los Alamos Laboratory. It is conceivable that Dr. Mark, or some other member of the Los Alamos Laboratory, may approach you with a request that you assist directly at Los Alamos itself. In this case I would like to advise you to consider such a request more seriously. The program of experimental and theoretical work going on at Los Alamos, in connection with which we are assisting on the theoretical end, is so important that anyone who is willing to assist, whether there or here, especially at this time, will be doing a great service."

Typical of the great number of "No" replies were:

- B: "Unwilling to give up University work for this type of defense work."
- C: Refused, working at Brookhaven, would get lower salary at Princeton.
- E: "Recently accepted position at Hughes Aircraft."
- F: "I do not wish to take part in the project you mention. The expectation value of the usefulness of my work is greatest on the hypothesis that I stick to my teaching job here."
- F: Continuing post-Ph. D. study abroad.
- G: Going to position with higher salary at Applied Physics Laboratory.
- K: "I believe that my development into an accomplished physicist would best be served by continuing here at H. in my present capacity."

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N: Desires to continue academic work.
S: "I cannot possibly come. I am unwilling to consider at this time contributing directly to our war potential."

Numerous responsible men were impressed by the national need and took part - some for six months, some for a year, and some for the whole 25 May 1951 - Spring 1953 period of the project. Th se who did come fell into two groups - young men who had just recently received their Ph. D's (or were in some cases in process of getting them; and a few older men with established positions who could secure leave of absence for the work. No one who was already well started on the early part of his career took part, except H. Pierre Noyes, whose help was much appreciated. In a number of cases better paying offers elsewhere were given up because of the feeling of urgency and patriotic obligation connected with the Matterhorn work. The membership of the project averaged 13 physicists, document custodian, secretary, three computers, and an IBM electronic computer staff of two. The associated computing facilities (IBM, New York and Princeton; UNIVAC, Philadelphia; and SEAC, Washington) greatly multiplied the working power of this group.

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This work was conducted in separate premises at the Forrestal Research Center. Adding a man to the staff at the date he could make, instead of at the uncertain date of his clearance, made the difference in several cases between getting the man and not getting him.

Within Princeton University, Project Matterhorn operated on a non-departmental basis under the jurisdiction of a management committee drawn from several divisions of the University, with Professor Lyman Spitzer, Jr., as chairman. Professor John A. Wheeler was scientific director of the project, and the associate director was Dr. John S. Toll. Though thermonuclear weapons or "burning" work (Division B) in the Matterhorn building at Princeton's Forrestal Research Center comes to an end in the spring of 1953, there will continue in the same building an entirely distinct project, under the scientific direction of Professor Spitzer, and under the auspices of the AEC Division of Research (Matterhorn, Division S), concerned with the desgin of a "Stellarator", or controlled thermonuclear reactor.

Computing Facilities

Before IBM equipment was available in Princeton, use was made of IBM facilities in New York. Later on even more extensive use was made of AEC-arranged computing facilities on SEAC in Washington and UNIVAC in Philadelphia. The staffs at these locations, under the direction of John Sheldon, Joseph Wegstein, Herbert Mitchell, Edward Cushen and Paul Chinitz, multiplied the effectiveness of Matterhorn personnel.

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Nature of Work

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Later work made the models more and more complicated -- but at the same time more accurate -- by taking into account simultaneously the maximum number of physical processes known to influence the burning significance. Further description of the way of work at Matterhorn is extracted from a letter written to an applied mathematician who wanted to know enough to decide whether to join: "There is then the problem of writing down the equations to describe that model. Some of the quantities which enter these equations are empirical functions. Various among us here have specialized in one or another aspect of the physics of our own problems, so that we are fairly well equipped with the empirical functions that we need for a wide class of problems. The equations once formulated, we would like to see how to get analytic solutions for them when possible. Usually this is not possible, and we have to set our equations up in a difference form suitable for electronic computation. There are people here who have had enough experience in this end so that they can take part of the burden in this respect. However, one has to pitch in on this to some extent himself in most cases in order to make progress at a reasonable rate and not to load up too heavily the ones among us who have had most experience with the electronic computer. Usually the most productive manner of participation, from the point of view of the Project, amounts to carrying on perhaps two things at the same time, one a project that may take from a few days to two weeks, and the other a longer term proposition which may take two weeks to two months. We hold regular Monday meetings to discuss the overall status of progress and to have everyone put forward his ideas, suggestions, and proposals for what ought to be done next. This democratic way of working puts on everyone a heavy responsibility to decide for himself whether he is working on the most important part of the whole thing within his reach. For this reason it is important that each of us barge in on the others for discussions, questions, and for the formulation of new ideas by discussion and argument. Of course, there are also special services that a mathematician can perform in our group: translation of an integral equation into a differential equation, suggestions how to 'solve' an otherwise complicated algebraic relation by a suitable parametric representation of the variables; advice about different methods of integration of differential equations as regards relative stability for large interval size. Here is a typical question of the service variety: .... But to pay attention to only such service questions would very greatly decrease one's usefulness. It is awareness of larger issues of the work and concern - - expressed by discussion with others and attempts to formulate and carry through idealized problems - - which make all the difference."

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"Briefly put, the work has in it lots of sweat and effort; there may very well be unclassified work coming out of it which you could publish; but this is to be considered purely incidental, accidental, and very much subsidiary to the main line effort; any major contact with one's normal scientific work is pretty much a nights and Sundays matter. Security restrictions are no bar to one's discussions about other matters or publications along purely scientific lines. If you are willing to face these rather heavy responsibilities, we would be very happy indeed to have you."

Three Phases of Work

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Ending of Project Matterhorn

The establishment of Project Whitney at Berkeley in the summer of 1952 created another center for work in the weapons field. This step, and requests from that new project for any theoretical talent willing to go to Livermore, forced reconsideration of the Matterhorn program. It was also apparent that Project Matterhorn had had since its beginning an emergency character which could not be continued indefinitely within the university set-up under peacetime conditions (heavy demands for overtime; work deadlines; lack of opportunity for research; $100/month or more short of salary for comparable work elsewhere). No solution for these difficulties being in sight, and a well defined phase of the thermonuclear assessment and primordial design work having been completed, it was concluded to terminate Project Matterhorn about 1 March 1953 despite the continuing need in the AEC program for some such center of analysis of long range weapon possibilities and of assessment of advantages of one weapon compared to another. This date was fixed as a compromise between the late 1952 date when Livermore wished to secure interested Matterhorn personnel, and the spring 1953 date for the freezing of the basic theoretical design of the Castle shots.

Nuclear Physics Reserve

The results obtained by Matterhorn are summarized in reports listed elsewhere in these pages. There follows a list of who made up the scientific staff of Matterhorn, and who by their experience and clearability now constitute in effect members of a U. S.

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Nuclear Physics Reserve (some of them still on active duty!):

Aron, Walter: A. B. University of Illinois 1942, M. A. 1944; Ph. D. University of California 1951; Matterhorn, June 1951 to March 1, 1953; DELETED Since March 1, 1953 with high energy accelerator project, Physics Department, Princeton University. DOE b(3)

Bellman, Richard: B. S. Brooklyn College 1941; Ph. D. Princeton University 1946, Professor Mathematics on leave of absence from Stanford University for Project Matterhorn, October 1, 1951 to April 8, 1952. DELETED Following July 1952 permanently with Rand Corporation, Santa Monica, California. DOE b(3)

Berger, Jay M.: B. S. University of Michigan 1947, A. M. Columbia University 1948, Ph. D. Case Institute of Technology 1952. Project Matterhorn, October 21, 1952 to February 28, 1953. DELETED With Stellarator controlled thermonuclear project, Princeton University, following March 1, 1953. DOE b(3)

Brown, Harold Dean: On loan from Argonne Laboratory - Savannah River Project, E. I. duPont de Nemours and Company, December 1952 to February 1953. DELETED DOE b(3)

Carter, David: B. A. University of British Columbia 1946, M. A. 1948, Ph. D. Princeton University 1952. Project Matterhorn, July 1951 to November 30, 1952. DELETED With T Division, Los Alamos, following December 1, 1952. DOE b(3)

Clendenin, William W.: B. A. Swarthmore College 1948, M. S. Yale University 1949, Ph. D. 1952. Project Matterhorn, July 1952 to February 28, 1953. DELETED With high energy accelerator project, Physics Department, Princeton University, after March 1, 1953. DOE b(3)

Driggers, Frank: On leave from Argonne Laboratory - Savannah River Project, E. I. duPont de Nemours and Company, October to December 1952. DELETED DOE b(3)

Ford, Kenneth W.: B. A. Harvard University 1948, Ph. D. Princeton University 1953. Project Matterhorn August 1951 to February 28, 1953. At different intervals during this period Dr. Ford worked only part-time, using the remaining portion of his time for work on his doctoral thesis. Analysis of all phases of thermonuclear burning. Los Alamos Scientific Laboratory July 1950 to July 1951. After March 1953 with high energy accelerator

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project, Physics Department, Princeton, part time and part time consultant to Los Alamos on SEAC calculations. After September 1953, staff, Physics Department, University of Indiana, Bloomington.

Frieman, Edward A.: B. S. Columbia University 1946, M. S. Polytechnic Institute 1948, Ph. D. 1951. Project Matterhorn, January 1952 to February 28, 1953. DELETED With Stellarator controlled thermonuclear project, Princeton University, following March 1, 1953. DOE b(3)

Goldgraber, Howard D.: B. M. E. The Cooper Union 1943, M. S. University of Pennsylvania 1946, Ph. D. 1952. Project Matterhorn, October 1952 to February 28, 1953. Hydrodynamic instability. With Project Whitney, Livermore, California, following March 1, 1953 with duty station at AEC UNIVAC, New York University.

Grasberger, William H.: A. B. University of California, 1950, M. A. 1951. Project Matterhorn September 12, 1951 to July 10, 1952. Opacity analysis; implosion and burning problems. Since August 1952 completing graduate work in astronomy, University of California, Berkeley.

Haefner, Richard: On loan from Argonne National Laboratory - Savannah River Project, E. I. duPont de Nemours and Company October 1952 to February 1953. DELETED DOE b(3)

Haussman, Capt. Alfred C.: Project Matterhorn, September 1, 1952 to February 28, 1953. DELETED With Project Whitney, Livermore, California following March 1, 1953. DOE b(3)

Heller, Jack: BaeE Brooklyn Polytechnic Institute 1946, MAeE 1946, Ph. D. 1950. University of Manchester, England, 1950-1951, University of Cambridge, England 1951-1952. Project Matterhorn May 1952 to March 11, 1953. DELETED Following March 11, 1953 with UNIVAC Center, Institute of Mechanics and Applied Mathematics, New York University. DOE b(3)

Henyey, Louis G.: Professor of Astrophysics, University of California, Berkeley, on leave with Matterhorn from August 1, 1951 to August 31, 1952. DELETED DOE b(3)

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Layzer, David R.: A. B. Harvard College 1947, Ph. D. Harvard University 1950. Project Matterhorn July 1952 to February 28, 1953. DELETED DOE b(3) Following March 1, 1953 with Solar Physics Project, Harvard College Observatory.

Levee, Richard D.: A. B. University of California at Los Angeles, M. A. University of California (Berkeley) 1951, Ph. D. 1951. Project Matterhorn October 22, 1951 to October 2, 1952. Opacity; steady state burning of deuterium and ignition thereof. Following October 1952 Assistant Professor of Astronomy, University of Missouri, Columbia.

McIntosh, John S.: B. S. Yale University 1948, M. S. 1949, Ph. D. 1952. Project Matterhorn, July 1952 to April 1, 1953. DELETED DOE b(3) Equation of state of deuterium compounds. Consultant to AEC thermonuclear work following April 1, 1953. Los Alamos (Summer 1953). After September 1953, staff, Physics Department, Princeton University.

Osborn, Howard A.: B. A. Princeton University, 1949. Project Matterhorn, September 11, 1951 to June 21, 1952. DELETED DOE b(3) Following June 1952 completing graduate work in mathematics, Stanford University.

Pennington, Capt. Ralph H.: Project Matterhorn, September 14, 1951 to December 20, 1952. DELETED DOE b(3) Following January 1, 1953 with Project Whitney, Livermore, California.

St. John, Daniel S.: On loan from Argonne National Laboratory - Savannah River Project, E. I. duPont de Nemours and Company, part-time October 1952 to February 28, 1953. DOE b(3) DELETED Assessment of design feasibility.

Toll, John S.: B. S. Yale University, 1944, M. S. Princeton University 1948, Ph.D. 1952. Los Alamos Scientific Laboratory June 1950 to August 1951. Project Matterhorn, February 1952 to March 1953. Associate Director of the Project from August 1, 1952 to March 1953. All phases of burning analysis and

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overall assessment. Following March 1, 1953 Chairman, Physics Department, University of Maryland, College Park, Maryland.

Wheeler, John A.: On leave of absence from Princeton University at Los Alamos, March 1950 - May 1951. Matterhorn, May 25, 1951 - April 1, 1953. DELETED After April 1, 1953, Physics Department, Princeton University. DOE b(3)

Wilets, Lawrence: B. S. University of Wisconsin 1948, M. A. Princeton University 1950, Ph. D. 1952. Project Matterhorn, November 1951 to February 28, 1953. DELETED Following March 1, 1953 with Project Whitney, Livermore, California. DOE b(3)

Compression

<u>Los Alamos Maniac IV Implosion Run</u>. The pressures assumed and the range of validity of the equations of state are applicable for radiation implosions only.

It is not presumed that these calculations should play the same role as the more detailed Los Alamos <u>SEAC</u> or <u>Maniac</u> implosion calculations, but rather it was hoped that, because they are essentially simpler and machine-wise faster, they could be used to survey the field and obtain an understanding of the implosion process as well as to acquire semi-quantitative information that could be used in optimizing design features. All calculations were carried out on the Eckert-Mauchly UNIVACs in Philadelphia.

The results of a number of problems are summarized in the following Table I.

In Table II are listed our best estimates of implosions attainable with standard Ivy geometry.

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Simplified Burning Analysis

calculations of burning that included all effects for which one knew how to make allowance, in order to make more or less justified predictions as to appropriate values of design parameters. However, there was always in mind the hope--not yet justified--that one might in addition construct (a) a simplified mock-up of the burning process and (b) from this mock-up derive a not too complicated formula for the yield of thermonuclear devices. It would be very desirable to have an equation analogous to the Bethe-Feynman equation for the yield of fission weapons. It is obvious such a formula must be considerably more involved in the thermonuclear case.

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For this reason it is perhaps understandable why it has not been possible up to now to construct such a yield formula.

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The six idealized problems that have been considered--though short of the kind of mock-up that is desired--do succeed in bringing out some of the factors important in burning.

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